The second division of the work is of a very miscellaneous character, ranging from quotations and anecdotes to a note on the superstition that thirteen is an unlucky number, and a remark that Newton's fluxions is not, as might be inferred from the title, a medical work. Any citation involving a geometrical term or a number seems to be regarded as worthy of a place here: thus the observation of a diplomatist that the straight course is not always the shortest way is regarded as a geometrical anecdote. Such examples abound, but it is an unusual stretch of language to call them mathematical.

The third division commences with some quotations which might have been equally well placed in the first part of the book. Most of it is, however, devoted to what are called paradoxes and singularities; such as a mnemonic verse for recollecting the approximate value of π, that the number six hundred and sixty-six is supposed to be connected with Antichrist, that a map of the heavens is now being prepared by photography, that two and two do not truly make four because the units used can never be exactly alike, that since it is very cold at the North Pole it might be thought that it was very hot at the South Pole, that Cardan's solution of a cubic equation is useless for determining real roots, and so on. Such a collection of assertions and extracts—mostly unaccompanied by comment or reference — will probably be less attractive to the mathematician than to the general reader.

The book concludes with some problems, classified according to subjects. In general, only the enunciations are given. Thus, in arithmetic, we have the question how many digits are used in paging a volume of one thousand six hundred and forty-five pages; in algebra, the question of finding the number of rabbits and pheasants when altogether among them there are thirty-five heads and ninety-four feet; in optics, the determination of a point equally illuminated by two given luminous points; in mechanics, the curve of pursuit, and in higher (!) mathematics, the question of finding the sum to which a centime would amount in eighteen hundred and eighty-nine years at compound interest at the rate of five per cent. a year. Some "recreations" and celebrated problems are also included: historical notes on these or references are either absent or so incomplete as to be practically useless, though they would add greatly to the interest of the questions. Moreover, it seems desirable to add a warning that questions of this kind—such as the inscription in a circle of a regular polygon of seventeen sides, the theory of the knight's tour on a chess-board, and the formation of perfect numbers—are of a totally different character to the common catch of the time occupied in reaching the top of a pole twenty metres high by a snail which each day crawls up three metres and each night slips down two metres. Here, however, conundrums and problems of all degrees of difficulty are indiscriminately mixed up together. In spite of this obvious criticism the collection is a good one, and well adapted to stimulate interest.

The printing and get-up of the book are admirable, while the foregoing sketch will, we think, enable the reader to form a general idea of its contents. We have already stated that, in our opinion, its chief value lies in the citations on the philosophy of the subject; the scientific worth of the rest is more questionable, but it forms an amusing collection of assertions and notes concerning mathematics which may be commended to those interested in such matters.

OUR BOOK SHELF.

Grasses of the Pacific Slope, including Alaska and the Adjacent Islands. Part II. By Dr. George Vasey, Botanist to the U.S. Department of Agriculture. Issued June 1, 1893. (Washington: Government Printing Office, 1893.)

THIS is the fourth part of a series of plates and descriptions of the rarer American grasses which has been issued by the United States Department of Agriculture. The first volume was devoted to the species of Texas and the south-west, and the present volume, which is now finished, contains plates and descriptions of the grasses of California, Oregon, Washington, and the north-western coast, including Alaska. Dr. Vasey, who for many years was head of the department at Washington, died before the present volume was issued. He was a native of Yorkshire, who emigrated to the Western States in early life. For many years Dr. Vasey lived in Illinois, and was one of the leading authorities on the plants of the Western States. He was already advanced in years when he became a government servant, and made a special study of the grasses.

The number of grasses of the Pacific Slope reaches nearly two hundred species. They are nearly all specifically distinct from the species east of the Mississippi River. A considerable number of the grasses of the mountain regions of California, Oregon, and Washington reappear in the mountains of Idaho, Montana, and the interior Rockies.

The interior of California is a dry region, verging in the extreme south into desert country, and is very deficient in grasses. In the present part there are descriptions and plates of 1 Schimdtia, 1 Phippsia, 1 Arctagrostis, 2 species of Agrostis, 3 of Calamagrostis, 2 of Deschampsia, 1 Trisetum, 1 Danthonia, 6 of Melica, 2 of Pleuropogon, 1 Uniola, 15 of Poa, 1 Colpodium, 2 of Dupontia, 1 Glycera, 1 Atropis, 3 of Festuca, 2 of Bromus, 1 Agropyrum, 3 of Elymus, and 1 Gymnostichum. Only two of the species are British, *Calamagrostis neglecta* and *Elymus arenarius*. The descriptions are mainly drawn up by Mr. L. H. Dewey. We wish the department could see its way next to issue a complete synopsis of all the grasses of the United States. J. G. B.

Reveries of World History, from Earth's Nebulous Origin to its Final Ruin; or, the Romance of a Star. By T. Mullett Ellis. (London: Swan Sonnenschein and Co., 1893.)

IT is difficult to estimate the place of this book in scientific literature. The object of its publication is apparently to give the world the benefit of moralisings very similar in style to those indulged in by Mr. Richard Swiveller of *Old Curiosity Shop* fame. But there is no substance in the book whatever. From the first to the last page the author meanders on with wearisome platitudes expressed in high-flaunting style, obscuring a line of fact in a page of padding. This is the manner in which he is carried away on the subject of the origin of man.

"He came like an apparition. Whence? Who shall say? Who shall dare, out of his absolute knowledge, to declare his origin, his ancestry?

"What hopes had he in that dreary desolation? What thoughts? What ambitions?

"What a being! What a nature, his! How noble his form! Between the mammoth and himself, between

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the animals that he chased and he, what a chasm in the scale of creation!"

So the rhapsody continues, without backbone or argument of any kind. Mr. Millett Ellis probably means well. Indeed, his literary effort may be highly appreciated by people who mistake verbiage for eloquence. To us, however, the contents appear to be in a nebulous condition like that which existed when, to use Mr. Ellis's words, "The chaos of earth circled in the vastness."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Publication of Physical Papers.

THE discussion started by Mr. J. Swinburne in NATURE of June 29, seems to have wandered from its original purpose, and from the points suggested in your article of July 13. It is the duty of an investigator in any branch of Natural Science to publish the results of his research if they appear to be both new and of sufficient interest. He has three courses open to him. He can write a book, relying on the advertisements of his publisher and on reviews to inform other workers that such a book exists. His space is unlimited, but he cannot make sure of a circulation unless by presentation copies. He may communicate his results to a scientific society. His space is somewhat limited, but he secures a definite circulation, and an opportunity for discussion. Or he can communicate them to some technical journal, securing generally the maximum circulation, but with considerable restriction as to space.

The publication of books needs no comment, and was alluded to by Mr. Swinburne. Mr. Buchanan thinks that the publication of papers in all branches of science is in as unsatisfactory a state as that of physics, and advocates the central organ suggested by Dr. Oliver Lodge, and referred to by Mr. Swinburne as a last resort. But the real question seems to be one of facilities for the publication of original communications, not of abstracts or reprints; and these not of general science, but of physics. Mr. Swinburne has given reasons which render a purely physical journal impossible, unless it be endowed or subsidised, as in the case of the excellent *Physical Review* of the Cornell University. Nothing could be more complete than the publication of chemical researches; and geology, astronomy, biology, and physiology, though untainted by the patentee, and treated often as a hobby rather than as a study, seem to be well provided for.

Exhaustive and important physical researches, such as those of Ewing on Magnetism, should undoubtedly be communicated to the Royal Society. The Transactions, it is true, are very expensive, and the circulation appears to depend largely on presentation copies, unless an abstract or reprint appears elsewhere. Work done at University laboratories is in many cases appropriately communicated to local societies, the circulation is almost negligible, and the majority of the papers are never heard of by outsiders. The Physical Society appears to offer the greatest facilities, and to publish its proceedings in the best possible manner—first through its own Proceedings, secondly by a recent arrangement with the *Philosophical Magazine*, thirdly by a number of reprints issued to the author, and last, but not least, by the admirable report which is sent to and printed by a number of technical journals, and which appear regularly in NATURE. Dr. Lodge thinks that "the method of publication invented, or at any rate adopted, by the Physical Society of London" is "well worthy of imitation." Not of imitation surely, for to multiply societies will hinder rather than facilitate reference to the published papers. The valuable work of the Physical Society needs extension. The double publication in the *Philosophical Magazine* and in the Proceedings seems to be somewhat wasteful. A selection of its heavy mathematical papers might be offered by the Society to the *Philosophical Magazine;* and thus leave room in its Proceedings for publication, in addition to its present contents, of papers accepted for printing, but not necessarily for reading and discussion; and for abstracts or for full publication of the more important physical papers of the societies of Cambridge, Oxford, Dublin, Edinburgh, and Glasgow. The Institution of Civil Engineers publishes a number of "selected papers"; these are either unsuitable for reading and discussion at a meeting, or are of minor importance. They receive perhaps less attention from London members who frequently attend the meetings, and who rarely read the Proceedings, but they secure as wide a circulation as those which are accepted for reading. A very successful departure was made by the Institution of Electrical Engineers in the publication, early in August, 1892, of a paper by Mr. J. Swinburne on "Problems of Commercial Electrolysis." At the meeting on November 10, this paper was taken as read, and a useful and vigorous discussion followed, occupying two evenings.

By the brief but careful and always accurate abstract of the papers, and by the no less excellent though much more care-demanding report of the discussion, an unusually wide circulation of the chief points of all communications to the Physical Society is secured. The lithographed reports are sent gratuitously to any technical journal that cares to publish them. Abstracts of "selected papers" could be sent out in the same way, and could be made with far less trouble, since, unlike the report which is issued a few days after each meeting, they could be dealt with at leisure; the abstract might indeed be furnished by the author, though I am not sure that this is advisable. The Physical Society seems in every way admirably fitted for the publication of physical papers, and no greater facilities for experimental demonstration could be desired than those which are so freely afforded to the authors of papers read before it.

Mr. Basset suggests that the London Mathematical Society would be a good medium for the publication of physical papers. May I protest against confusion between the two sciences, and I am sure that many pure mathematicians, though from a different point of view, will agree with me? There was once and I believe still exists, a "science" called Microscopy. It was divided into two factions, strongly opposed to each other. These were named, reciprocally I suppose, "The Glass and Brass School," and "The Slug and Bug School." The one regarded the *navicula* and the *grammatophora* as having been specially created to afford test objects for showing off the performance of favourite instruments; the other considered immersion objectives and correcting adjustments as mere tools, the use of which must be learned; and regarded polariscope attachments and mechanical stages as toys. The problem worker, it would be ambiguous to call him a mixed mathematician, finds in physics an unlimited material for problem-setting. In many cases he accidentally makes an original contribution to physical science, he frequently gives most valuable finishing touches to the experimental work of others, and occasionally actually retards progress, as in alternate current electrical engineering, by introducing complications on insufficient data, to the discouragement and confusion of the experimental worker. Good tools are in themselves a delight to a good workman; no true physicist can disregard mathematics, even in those branches which are to him useless. But the aims differ, and the sciences should be distinct.

There need be no rivalry whatever with the Royal Society. Elaborate memoirs do not admit of discussion, and it would be a pity to burden the Physical Society with the expense of printing them, when the Royal Society can so well afford to give them place in its Transactions. The strict rules of the Institution of Civil Engineers against the publication of its papers in technical journals before they have appeared in its own proceedings, and the similar rule of the Royal Society, so far as its Transactions are concerned, may perhaps be in keeping with the unapproached prestige of these institutions. But the Royal Society issues proofs of its Proceedings after the papers have been read, and the Institution of Mechanical Engineers and other societies send out proofs of their papers with the simple condition that they are not to be reprinted until the communications have been actually read. The Physical Society tacitly adopts the latter course, sending proofs before the meeting to those who are likely to take part in the discussion. The only objection to the free publication of a paper *before* it is read and discussed, is that a discussion may take place prematurely in the technical journals; but with the exception of NATURE, these are in touch with so few branches of physics, that but little harm would be done.

I venture to think that the publication of physical papers can be carried out most efficiently by means of the present procedure

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